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Dear Josh:

Perhaps the best way to answer yours of April 26th is to say, yes, in the best of all possible worlds there would still be an extensive genetic program in Hiroshima and Nagasaki, employing several geneticists and an even greater number of pediatricians. You are not t the first to suggest that there should be a much more active program in progress at the present time. Unfortunately, this is the kind of a project everyone is all for as long as he doesn't carry the primary responsibility. I will not bore you with the problem of recruitng geneticists and ancillary personnel with which I struggled for some eight years. Particularly in the early years, when life in Japan was much tougher than what you have just witnessed, it was almost impossible to find participants.

However, I am not sure that your informant during the course of your visit gave you a fair picture of what continuing genetic activity there was. Firstly, we do continue to collect data on the sex ratio effect, which has emerged as the most promising of the few leads concerning tangible genetic effects which came out of the study. Secondly, there is in progress right now a study on the death rate amongst the F_1, a study which, if we can extrapolate from Bill Russell's results with mice, as well as those of Lush and Hazel with swine, might just show something. Thirdly, our consanguinity studies (of which a first report is in the current PNAS), which were a direct outgrowth of the old genetic study, are continuing, and we are in fact right now planning a return trip to Japan to clean up some odds and ends there. In this connection, I would say that from the scientific standpoint the yield from these latter studies, which were only possible because of the preceding study, is really very high.

With respect to the comment on the "appalling" follow-up on congenital anomalies, I am not quite sure what you have in mind. As we pointed out in our 1956 opus, our total number of congenital defects checks out very well with the results of others. With respect to the specific question of

mongolism, I wonder if you have seen our little note in Lancet, Mar. 10, 1962, pp. 537-538? Following the article by Uchida and Curtis we re-examined our data and were quite unable to confirm their report. In the 15,000-odd children re-examined at age 9 months, we had a frequency of mongolism of 1 per 1,000. This is lower than what we would expect in a Caucasian population, but it's the best series available for Japan at the moment; it doesn't look as if we did too badly. As a matter of fact, we feel that if Uchida and Curtis had stopped to reflect about the probable dosages involved in their material, and the implications of their apparent increase, they would have been much more cautious. At any rate, as you will see from this note, we are thinking about the relationship between non-disjunction and radiation. In this connection, I have just sent off the manuscript for a little monograph on radiation effects, which examines rather extensively what nondisjunction might do to the sex ratio; I am also at this moment in correspondence with the ABCC about the possibility of surveys on Hiroshima and Nagasaki school children, trying to relate the occurrence of nondisjunction types to radiation history. But as is so often the case in population work, this is a large decision, tying up three or four people for a couple of years.

Finally, I agree with you that there is an opportunity for population genetics being missed in the overall program of the ABCC. You are not the first to point that out. We too have made the point. But faced with the same difficult choices regarding time utilization as any other investigator, I really haven't felt it was worth the two years of work it would take to revamp their records and their thinking. Any volunteers?

Best regards,

James V. Neel, M.D.

JVN:kw